Cannon, Richard O. 2019

Dr. Richard O. Cannon III Oral History

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National Heart, Lung, and Blood Institute Oral History Project

Interview with Richard O. Cannon III

Conducted on July 16, 2019, by Sheree Scarborough

Biographical Statement

Dr. Richard O. Cannon III was born in Nashville, Tennessee, in 1950. He received his Bachelor of Science degree from Vanderbilt University in 1972, and his medical degree from Vanderbilt University School of Medicine in 1976. He completed residency training in internal medicine at Barnes Hospital (Washington University School of Medicine) in St. Louis, Missouri, in 1979, and subsequently came to the National Institutes of Health for cardiology fellowship training at the Clinical Center and stayed for forty years. Dr. Cannon was appointed Clinical Director of the National Heart Lung and Blood Institute in 2002, and served in this role until 2012. He also served as head of the Clinical Cardiology Section in the Cardiovascular Branch and as clinical professor of medicine at the Georgetown University Medical Center. In 2012, he became Chair of the Institutional Review Board within the NHLBI, a position he held through its merger with NHGRI and the subsequent consolidation of the intramural IRBs. Dr. Cannon's research in the field of clinical cardiology has focused on hypertrophic cardiomyopathy, coronary microvascular ischemia, nitric oxide transport in blood, and the role of nitric oxide deficiency in diabetic vascular disease. He holds several patents along with Mark Gladwin that involve the medical applications of nitric oxide and its oxidized products. Dr. Cannon has authored or coauthored over 200 publications, and has been honored by the U.S. Public Health Service; the Medical and Surgical Society of Bologna, Italy; and the Fukushima Society of Medical Science of Japan. He has also received numerous awards within the NIH, including the NIH Clinical Fellows Teaching Award, the NIH Director's Award, and most recently the Office of the NIH Director Administrative Award for IRB Operations and Reorganization (2019).

Interview Synopsis

Dr. Cannon begins the interview with memories of growing up in Nashville and memories of his father who was a medical doctor and Director of Vanderbilt Hospital for many years. He remembers choosing to major in physics and astronomy, unusual majors for a premed student, at Vanderbilt, and going on to medical school there. He discusses some aspects of his medical training at Barnes Hospital at Washington University School of Medicine in St. Louis, and his decision to come to the NIH after meeting Gene Passamani, who was a senior resident at Washington University after spending two years as a research fellow at the Clinical Center. Intending to come to the NIH for a two-year stint for a research-training program, Dr. Cannon spent the next forty years there. He discusses changes in the field of cardiology and as practiced at the Clinical Center over the time period he has been at the NIH. He also speaks about the three main research focuses of his research career: hypertrophic cardiomyopathy, microvascular angina, and nitric oxide, as well as his successful stint as IRB Chair. In addition, Dr. Cannon generously talks about those who he considers mentors, including Stephen Epstein, Bob Bonow, and Kenny Kent. He also mentions collaborators, including Julio Panza, Arshed Quyyumi, Mark Gladwin, and Anne Sumner, and the thirty fellows he has mentored over the years. One of his proudest accomplishments is the MobileMed Heart Clinic that he helped establish twelve years ago for local residents without insurance who need cardiac care. Dr. Cannon concludes the interview with his thoughts about what it means to be a good doctor and his wish to be remembered as one.

SS: This is Sheree Scarborough and I'm here at the Clinical Center on the campus of NIH. This is an oral history with Dr. Richard Cannon III for the NHLBI Oral History Project. Today is July 16, 2019. Dr. Cannon, I believe you were born in Nashville?

RC: I was, in 1950. Nashville was at that time a prototypic provincial southern city with statues of the Confederacy all over the place and very much in tune with practices now that we look back upon with some shame and regret, but that was the environment I grew up in, from an upper middle class family. My dad was a doctor, so from an early age I knew I wanted to be a doctor. I didn't go through a phase of wanting to be a fireman or anything else. When people asked me, "What do you want to be when you grew up?" I said, "I'm going to be a doctor like my dad." So that's what I did. I went to high school then went to college, Vanderbilt University in Nashville, and then entered Vanderbilt Medical School in 1972.

SS: Your father was a doctor and was in fact associated with Vanderbilt, is that correct?

RC: He was. His background was OB/GYN, but shortly after he completed his training he was drafted in the Army and served in the Korean War, but served stateside. So our young family, in the early 1950s, was uprooted from Nashville where I was born, to San Antonio, Texas, where he was stationed at Fort Sam Houston Brooke Army Hospital. While he was there he had an opportunity to learn about hospital administration. He found that he really liked that, so when his tour was over, and he returned to Nashville, he was offered a position of director of Vanderbilt Hospital. I think he was assistant director for a couple of years, but very quickly became director of Vanderbilt Hospital. And that's what he did through his adult career.

He wasn't a practicing physician. He was a hospital administrator, but still he was a doctor in my eyes and that's what I wanted to be. My grandmother would tease me and say, "You're not like your dad. You want to be a real doctor." That drove my dad nuts. It was actually my mother's mother who said that, his mother-in-law. I don't think his mother would say that.

SS: How many siblings did you have?

RC: At that time there were three children. We picked up my younger brother in San Antonio. He's the sole Texan. The rest of us are Tennesseans. Four children. There are three of us left. My oldest sibling died a couple of years ago, so there are three of us left and we're very close.

SS: Do you have memories of that time in Texas?

RC: I was just a couple of years old. I was born in 1950 and we left [Nashville] in 1951 then came back in 1954. I have just faint snippets when I was approaching four years of age of living there. It's interesting, it was about fifteen years ago, my mother had passed, and my dad was living in an assisted living facility near where my sister lives in Tyler, Texas. The rest of us thought it would be fun to have a family get-together. My wife and I have two daughters and my brother's family, at that time, he was living in New Jersey, and my sister living in Texas, another sister, the one who has since passed, she was living in Atlanta. We thought it would be great to get together with dad and go back to San Antonio.

I had been to San Antonio for a couple of meetings over the years, but not really had an opportunity to see where we lived. So we go there and one of the first things we did, this is twenty-something of us that had all gathered together, we wanted to see where we lived in San Antonio. Dad said, "I remember the address." We went out to Funston Place near the Fort Sam Houston campus and we found our little house, and it was really a small house. I was amazed that all of us could fit in that little house. Dad said, "That's where we lived."

But it was fun. Dad got teary-eyed when we got on the base. He pointed out, "That's where I worked at that time." Brooke Army Hospital was actually on this big parade ground. Now there's a modern hospital completely separate from the parade ground. Dad remembered parading up and down the parade ground and going to work at Brooke Army Hospital. That was a lot of fun, very nostalgic for my dad.

SS: Did he die soon after that?

RC: He did. He passed maybe five years after that.

SS: You remember your childhood in Nashville more than in Texas?

RC: I do because we came back to Nashville. I went to elementary school then to high school, then to college. Then I followed my dad's footsteps and went to Vanderbilt Medical School, not that I necessarily did that to please him, but that was the best school I could get into. I also met my future wife in college. She went to Vanderbilt as well and we were married my first year in medical school. Her name is Gail. Her maiden name was Bentley. She was born in California but then her family settled in Oklahoma. She was in nursing school when I was in arts and sciences at Vanderbilt. We met our senior year and we were married my first year in medical school.

After four years at Vanderbilt, which I enjoyed very much, I think they prepared me well for my future, I decided it was time to leave Nashville. I had been in Nashville for a long time and I wanted to see the world. That's when I applied for internal medicine residency programs and I matched at Barnes Hospital, Washington University School of Medicine in St. Louis, Missouri. That's where I did my medicine training from 1976 to 1979.

SS: You said you wanted to be a doctor from the time you were a small boy from idolizing your father?

RC: Yes, I think so. He was highly regarded in our community and carried himself well, and I was impressed with his depth of knowledge. Even though he didn't wear a stethoscope or carry a little black bag with him, even though he was more on the administrative side of things, I was impressed with his bearing, his prestige and reputation in the community, and I just thought, “Yes, that's what I want to do.” It was at such an early age I didn't really consider other options. People would ask me, "What are you going to do when you grow up, Richard?" I just assumed that's what I was going to do. I had never really considered other options.

SS: It’s great you were able to, not everybody can get into medical school.

RC: That's true.

SS: And your undergraduate degree was in physics and astronomy?

RC: It was. It's an unusual major. Most of the pre-meds at Vanderbilt, as was true for most schools at that time, were biology majors or chemistry majors. That was the traditional pathway. I can remember some of my early courses. Obviously, I had to take courses obviously in chemistry and biology, and they were all pre-meds and they were all very anxious and nervous, and high strung, and I said, “I don't want to hang around with these people.” I had to take physics as well and I found them to be a little more calm. There weren't so many pre-meds in physics. In fact, I don't know of any other pre-meds, but certainly not like chemistry and biology. I thought this is a little calmer and I think I'll go this route. I still had to take the required courses in chemistry and biology, but I just didn't have to hang around with those folks quite as much. I don't know that it was necessarily easier because it had a lot of mathematics and I'm not a very strong math student. Anyway, I could understand enough math to get through physics.

And then astronomy was just a couple of extra hours. It was fun for me because Vanderbilt had an observatory about fifteen miles from the campus, up on a high hill. So I had an opportunity to use the telescope, it was a 24-inch reflecting telescope. Actually, my first publication was an astronomy publication while I was in college.

SS: Did that almost make you want to veer off course?

RC: No, I was still going to be a doctor. They understood that. In fact, I think they were intrigued to have a pre-med student in physics. I think they took a different interest in me knowing that I wasn't staking my career on physics. Maybe they felt it's good that he has another goal in sight. Physics is probably not for him.

SS: Then you get to Barnes Hospital for your internship. Was that in cardiology?

RC: No, that was in internal medicine. As is true for all post-graduate education, you first have to pick a specialty. Internal medicine is a common specialty after graduation from medical school. Then after that you choose a subspecialty such as cardiology. But at that time I really didn't think cardiology was what I wanted to do. Vanderbilt had a very strong program in endocrinology and I enjoyed memorizing all of these endocrine disorders, the testing for endocrine disorders, the unusual presentations that some of these patients had, and I thought: “That looks interesting to me.” It wasn't really until I got to Barnes Hospital which had a strong cardiology program that I thought, “I think this is what I want to do.” But you had to do internal medicine first.

From 1976 to 1979, I did my three years of medicine training at Barnes. I enjoyed it very much. My wife and I enjoyed living in St. Louis. St. Louis doesn't have a great reputation as a livable city, but for the brief time that I had outside of the hospital, we found interesting things to do. At that time internships and residencies were very time consuming, much more than nowadays. Currently, there are regulations that limit the number of hours that an intern and resident can spend in the hospital. At that time, however, there were no such restrictions and the thinking was the longer you stay in a hospital the more you're going to learn. That was the philosophy and that's why house staff are called house staff; they're in the house. If you're away from the house you're missing an opportunity to learn something, to understand disease and take care of patients. That was the philosophy.

My wife and I didn't see very much of each other. She got a job as a nurse in the neonatal intensive care unit at the [St. Louis] Children's Hospital. But we did manage to have one child, so our older daughter was born in my senior year when I had a little extra time on my hands. Her name is Jennifer and she's also a doctor. She also went to Vanderbilt, so there are three generations of doctors that graduated from Vanderbilt. She graduated in 2005. They allowed my dad and me to be up on stage with her, and to give her the diploma. That was fun to have three generations of doctors, and that moment was very special for my dad and me.

SS: So she idolized *you*?

RC: Well, I certainly did not pressure her to be a doctor and she did consider other things. So it's not like she just had this gene that directed her to become a doctor and nothing else. She thought about other things, but that's ultimately what she decided to do.

SS: What is her specialty?

RC: Pediatrics. She is a general pediatrician and her husband is a pediatrician, so they're a two-pediatrician family. They have two children, our two older grandchildren Anna, age eleven and Ryan, age nine.

Then our younger daughter Leah who I think looked at what I went through in my time spent at work and my older daughter went through in her studies and said, “I don't want to do that.” She is not a doctor, and she and her husband live south of Atlanta. She works in Atlanta at Neiman Marcus, where she manages the jewelry department. In some respects she's probably more successful than my older daughter who’s the doctor. And they have a child Hilary, age five. So we have three grandchildren.

SS: Where does your older daughter live?

RC: Nashville. They're back in Nashville.

SS: Tell me about the Barnes Hospital.

RC: Barnes Hospital is the major teaching hospital for Washington University School of Medicine. At that time Barnes was a huge sprawling hospital and next to it was a smaller hospital called Jewish Hospital. They had separate training programs with separate internships, separate residencies. With their subspecialty training there was overlap, but for internal medicine training, they were completely separate. Occasionally I would go over there for a conference or something. We stayed apart until we would merge together when we had joint rotations at our City Hospital and at the VA Hospital. Now they've since merged, so now it's Barnes-Jewish Hospital.

There is another medical school in St. Louis, St. Louis University School of Medicine and they have their own teaching hospital. They're a little bit further downtown from the Barnes Hospital complex. We would join them at the VA and the City Hospital, although we had separate units, but we'd get together for conferences. I don't know how it's organized now. It may be different. But at that time there was the Washington Unit and there was a St. Louis U Unit—very inefficient, but that's the way it grew. They didn't want to pool us all together. Both medical schools wanted to have access to a city hospital population and a VA Hospital population, so that's what was done.

SS: St. Louis is colder than Nashville.

RC: Yes. Colder, much more snow. There would be snow on the ground from November until early March. In Nashville when we had an inch of snow, it would shut down the city. Much more snow, much colder in St. Louis.

SS: How did you respond to that?

RC: I was in the hospital all the time. I didn't get out. Obviously, I'd go home sometimes. It was an adjustment, but it was different. I had been in Nashville for so long I really wanted to go someplace else. There were people in Nashville that wanted me to stay and be chief resident there, but I just felt like I needed to get out and prove myself elsewhere, do something, so I did. I'm glad I did. I think it was good for me because there were some practices at Wash U different from what I was taught at Vanderbilt, not that one is necessarily better than the other. It sometimes reflects ignorance of the best thing to do. But you see different approaches and different ways of doing things, so I was very pleased I went there. And then when I was at Wash U at Barnes Hospital that was where I met Gene Passamani who was responsible for me coming to the NIH.

SS: Before that you moved on to cardiology?

RC: No. Cardiology was done here.

SS: How did you get to NIH?

RC: When I was a junior resident, the second of the three years of my training in medicine, I was wondering what to do. As I told you, I initially thought I wanted to be an endocrinologist, but I was really attracted to cardiology. I was on a rotation with a new senior resident who had just come from the NIH, Gene Passamani. He had gone to medical school in Michigan, but then went to Mass General for medicine training and then he went to NIH for research training. Then he wanted to be a cardiologist, so he came to Wash U for his cardiology training, but first he had to do a senior residency here. It's complicated because you have to have a certain number of years before you can proceed onto subspecialty training. He did two years at Mass General and he still had one more year he needed to complete to go into cardiology. That's the way it was at that time.

I was assigned to him as a junior resident. He told me about the NIH. I didn't know much about the NIH at the time, but he convinced me that if I had any interest in doing research this was a good place to come. At that time it was not that long after the Vietnam War. During the Vietnam War if you were interested in research this was a good place to come because it got you out of military service. You could come as a Public Health Service officer and that would satisfy any military commitment. A lot of very bright people came to the NIH during that time. He was the beneficiary of that expertise and really had a very good research experience here and in fact planned to go back.

We got to talking and he knew I was not really sure what I wanted to do. So he said, "You might think about just taking a couple of years if you're in no hurry and you're still wondering about what to do, go to the NIH." I kicked that around and I talked to some people at Barnes. There were mixed feelings because some of them said, "Stay here, you don't need to go to the NIH. You want to do research, we do research, you can do research here." Gene would say, "You should get a different perspective just like you did when you went from Vanderbilt to Barnes and Wash U Medical School. It's good to get different perspectives. They're so good at the NIH, they're so smart, you really ought to go there."

I applied and I was invited for an interview, I was accepted, and here is where I came. The understanding was with Wash U that after I spent maybe two years here I would go back to Wash U as chief resident and then into cardiology there. I had it all mapped out. I'd spend two years there and it would fun to live in Washington and then we'll go back. That was the agreement.

I came here in July 1979, forty years ago. I was in a research-training program that no longer exists, but it was for learning clinical research. It involved one year of basically taking care of patients that were here for research protocols then after that choosing a research field that you would go into for the next year. That was a little bit of a disappointment in that I had done three years of medicine training and here I was caring for patients almost like an intern. On the other hand, I was exposed to some interesting patients that I had never seen before or rarely had seen in my training in my three years at Barnes Hospital.

I was particularly attracted to the cardiology service here. I was leaning towards cardiology and had an arrangement to return to Wash U and do my cardiology fellowship training there. Then I get here and I see these interesting patients. At that time the cardiology program was very exciting. We had a broad spectrum of cardiology expertise. There was an active heart surgery program here at that time. It no longer exists. Things have changed considerably since that time. I felt like I was in a super cardiology program and I ended up staying here. I did not go back to Wash U. They were disappointed, but wished me well and I've stayed here for forty years. That was not my initial intent.

SS: Why were the patients more interesting here?

RC: Because they were here for research protocols, so they all had something exotic, something very unusual about them. It wasn't like Wash U where I would be caring for patients who had garden-variety cardiology problems or other kinds of problems, but were not participating in research protocols. They were there to be cared for, and I learned a lot about a broad spectrum of diseases and management of those diseases. Here there was more of a focus on certain kinds of problems that were very unusual and they were all here with the intent of being in a research protocol.

At that time the heart disease that was of particular interest was hypertrophic cardiomyopathy. At the time it was believed to be very rare, now it's known to be not that rare. One in every 400 to 500 people is born with this condition that affects heart muscle and is genetically based so it tends to run in families, and it has very unusual hemodynamic characteristics.

At that time that I came in 1979, cardiologists here were interested in those hemodynamic characteristics and there were some drugs that were being investigated that were believed to be useful. Further, there was an operation that had been developed by Glenn Morrow, who was the head heart surgeon here, for a subset of those patients that I had never heard of before. This was exciting to see these patients with interesting physical findings and interesting hemodynamics that could be measured in the cath lab, and interesting approaches to treatments that were completely novel to me. That was very exciting. There were also people here that were specialists in echo and imaging, and in cardiac catheterization. Then we had our heart surgeons. I thought, “This is an exciting place to be, I think I'll stay.”

What made it easy for me to stay is that at that time it was possible to get an exemption for some parts of cardiology fellowship training to be board certified in cardiology. They recognized this as a program acceptable for full cardiac fellowship training. So I could stay here and be involved in clinical research and get my cardiology board certification as well. That's no longer true, but at the time it was and that's what I did.

SS: So you could kill two birds.

RC: Exactly right. Plus we had a second child, one born here across the street at what used to be called Bethesda Naval Hospital, because I was a Public Health Service officer. That's the way most physicians came at the time. We had two children in tow. It just seemed to be the right thing to do at the time.

SS: Did your wife go back to work?

RC: No. When our first child was born she stayed home with our daughter, Jennie, who is now a pediatrician. Soon after we moved here and we got settled in, she was pregnant with our second child. Before she was pregnant with our second child she did want to go back to work and actually had a job lined up at Montgomery County General Hospital then she found out she's pregnant with child number two, Leah, the one who lives in Atlanta. After Leah was born we agreed that it was time to take some time off and raise those two children. That's what we decided to do.

We struggled with my one income, which wasn't very much at the time at the NIH on the Public Health Service pay scale. But we survived. After our children were five and six years of age she had an opportunity to work at the elementary school where our older daughter was in school, as a media assistant. My wife is very computer savvy. In the old days they called them librarians, but now they call them media specialists, because it's not so much books but computers for information. She worked in two elementary schools as a media specialist.

SS: Okay, I wondered if she worked here?

RC: No. That's what she did until she retired from the Montgomery County Public School system. She never returned to nursing, but she was very happy with her career and liked working with kids and with computers, and helping kids with their lessons, and that's what she did.

SS: Gene Passamani, would you call him a friend or a mentor?

RC: Both. He still is a friend. He came back to the NIH a year after I did because he stayed for his cardiology fellowship. I came here into this clinical research-training program. He completed his cardiology fellowship training and then he came back to the NIH and was actually director of the extramural division of cardiology for a number of years. That's the division that funds NIH-sponsored clinical trials, and his division, obviously cardiology focused. Even though he was in a different building we still crossed paths on occasion and kept up over the years.

He was very much involved—now I'm going to jump ahead, but we can return to it later on—in starting a heart clinic [MobileMed Heart Clinic] across the street at Suburban Hospital for people who don't have insurance. If you were to ask me what are my proudest accomplishments in my four years at the NIH, one of them would definitely have to be starting that clinic along with Gene Passamani, which is still in existence. We've been in existence twelve years this October. It's held on Thursday evenings and we are allowed to meet at Suburban Hospital. The hospital provides us with some rooms to examine patients. It's under the auspices of Mobile Medical Care, a non-profit organization that provides care to homeless and uninsured residents of Montgomery County. They have buses that go out to homeless shelters and they also have clinics that meet in various places with volunteer primary care providers and staff, and there was a need for specialists care for patients with suspected or known heart disease.

Prior to that, MobileMed would just beg cardiologists, "Please see my patient who has this loud heart murmur or is complaining of chest pain," and that's not a very efficient or effective way to care for those patients or they would wait until there was some calamitous event and they'd go to the emergency room and then the hospital would have to care for them at least for a while. Obviously, that's not a great way to care for patients.

Gene and I started this clinic. Gene came to me thirteen years ago. At that time he had left his job at the NIH and he was working at Suburban Hospital as head of their medicine program. They were starting a heart surgery program. He thought this would be an ideal time for us to join forces and set up this clinic with the possibility of providing surgery care because many heart patients do need surgery in time. We started that clinic. Gene never participated as a cardiologist. He left that a long time ago. But was one of the administrative forces behind establishing this clinic.

I still go every Thursday evening. I walk across the street to Suburban Hospital, which is now owned by Johns Hopkins Medical. I'm very pleased with our clinic. Our patients are appreciative of what we do. There are other volunteers as well from the NIH and some from the community who volunteer there. That's been a lot of fun and I'm very proud of that.

SS: Who were some other mentors for you?

RC: Gene was interested in getting me to come to the NIH. Once I got here my primary mentor was Stephen Epstein. He was the head of the cardiology branch, an outgoing individual, and very accomplished in clinical research. The program was clinically oriented at that time, so we were focused into hemodynamics and imaging. There was a basic research component, but that was a fairly minor part of the branch activity. Now it's the opposite. Now it's a major part of this department. Most of what we did, what existed in the division at that time, in the cardiology branch, was physiologically or pathophysiologically oriented. It involved research done with actual patients, so I found that exciting.

Steve Epstein had come to the NIH at an early age in his career during the Vietnam War. Remember I told you about the best and the brightest coming to do research. The Vietnam War ended in 1975, and the draft ended as well, so there was no longer this attraction to avoid military service by serving in the Public Health Service. He came with that group of very bright people. He saw that I was interested in cardiology and my background at Barnes Hospital, and my plan to go back to Wash U and do my cardiology training. He said, "Just stay here. You don't need to go back. You can get your cardiology training here and learn research, and get a lot of great things accomplished." So that's what I did. He was my mentor and I still consider him my friend. He left here a number of years ago. He moved to work at Washington Hospital Center, but I don't know if he's still working there. We've lost touch the last couple of years. I still consider him my guiding mentor and friend during those early formative years.

SS: It sounds like an interesting atmosphere.

RC: It was great because we had it all then. We were much like a cardiology department that I was used to at Barnes Hospital but smaller, not as many people, not as many patients. We had our own inpatient unit in the old Clinical Center. We had an intensive care unit, we had cath labs, we had imaging resources, we had heart surgery here. We had what I was used to, what I was accustomed to at my training at Wash U but on a smaller more research-focused scale. It was like having a research center in a hospital. And the Clinical Center, at that time, was much like what I was accustomed to at Washington University. Most of the specialties were represented and the place was full of patients. It wasn't that much different from what I was used to at Wash U, only it was researched-focused, whereas Wash U was more patient care oriented.

SS: Tell me about your teammates and colleagues.

RC: There were people that I met shortly after coming here, some of whom were already here, that became mentors as well and they moved on to prestigious opportunities. Bob Bonow [Robert O. Bonow] comes to mind. Bob Bonow came a few years earlier to the same research training program that I came to. He did his medicine training at University of Pennsylvania. He gravitated to nuclear imaging and became accomplished at that, then ultimately became Chief of Cardiology at Northwestern University School of Medicine in Chicago, where he still lives. Our careers overlapped for about ten years, during which time he was an important mentor to me in clinical research. We did a lot of collaborations, many of which were in the hypertrophic cardiomyopathy field. He was interested in imaging, understanding the pathophysiology using imaging techniques for this unusual cardiomyopathy. We had a lot of collaborations that I thought were a lot fun, very interesting, and resulted in several important publications.

Another mentor was Kenny Kent. He was the head of the cath lab and he was the first person in the Washington area to do cardiac angioplasty with inflatable balloons on catheters positioned in coronary arteries. Nowadays, angioplasty is common putting stents in arteries to open them up. There was no angioplasty when I was at Wash U and even when I first arrived here it hadn't been described by Dr. [Andreas] Gruentzig. Dr. Gruentzig, who was in Switzerland at the time, had this idea based on earlier more primitive procedures to open up arteries to use a device attached to a catheter to dilate a blockage. Then years later other people refined coronary angioplasty by putting metallic stents to keep arteries open. Gruentzig did the first procedure in Switzerland. Kenny Kent hopped on a plane, went over to Switzerland, watched him do it, comes back here and he does a few right here at the NIH, and I got to watch him do it. I thought, “That's really exciting, I want to learn how to do that,” and ultimately he did teach me how to do that. That was fun. Kenny Kent left to go to Georgetown University and then the Washington Hospital Center around 1985.

Another person who was in the cath lab at that time who has now come back to the NIH is Doug Rosing. Doug is a clinical cardiologist like I consider myself to be. He and I continue to be friends and colleagues. Now he directs the Consult Service here on which I serve a couple of days a week. I consider that to be one of the highlights of my career, serving on the Consult Service, because I get to do clinical cardiology, which is what I enjoy doing.

SS: You're consulting with patients?

RC: Yes. We see patients throughout the Clinical Center who come here to participate in research protocols. Many of them either come with a cardiac condition or they acquire a cardiac condition either as a result of their participation, such as toxicity of a drug that was given, or independent of participation and happened to have a heart problem while they were here. I enjoy doing that because although the problems we see are fairly common heart problems, they're in an unusual setting and context, which can make them challenging to manage at times.

SS: It sounds like there was a lot interesting new things going on during the early 1980s.

RC: The early 1980s, I was still in my formative years because I came in '79. I didn't complete my cardiology training until '83. At that time I had the opportunity to come into the cath lab. Kenny Kent and Doug Rosing were my teachers in the cath lab. I learned to be an invasive cardiologist and subsequently learned how to do angioplasty under Dr. Kent.

As far as research is concerned, initially my research was focused on hypertrophic cardiomyopathy, this uncommon, but not rare, heart muscle disorder. It has interesting and unusual clinical findings and treatment considerations. At that time there was controversy as to how serious a problem this was and how to best treat it, particularly with surgery. There was a surgeon here who invented this operation that I had not heard of before coming here, Dr. Morrow. There was controversy as to whether this operation actually worked and made physiologic sense to do it.

My initial years here were in the cath lab making lots of different measurements in these patients, such as heart chamber pressures and blood flow and looking for evidence for insufficient blood flow or ischemia. That was great for me, because I had an opportunity not only to learn cardiology, but also to engage in clinical research. So very quickly I was able to get some things accomplished and some papers written, and started making presentations for grand rounds at other medical centers, and be invited to conferences in foreign countries. For a thirty-something year old guy this was pretty exciting stuff. That was my high watermark for that phase of clinical research.

Then people started to leave. Dr. Rosing left to go into private practice. Dr. Kent left to go to Georgetown. Dr. Bonow became chief of cardiology at Northwestern in Chicago. And some of the more exciting things in hypertrophic cardiomyopathy were now being done elsewhere, particularly the genetics of the disease. Further, the heart surgery program closed and Dr. Epstein left for the Washington Hospital Center.

In the late 1980s, I felt it was time to move into a different area, and the area in which I got interested was in patients who were referred to us with suspected cardiomyopathy or suspected heart disease often presenting with chest pain, but yet all of our testing would not show that they had cardiomyopathy or had a blockage in their arteries. I got interested in whether or not these people had a cardiac problem at all, in particular for those that we believed did. There was some evidence that they had heart problem, but we couldn't determine whether it was a problem with blood flow in the heart. That occupied ten years or so of my career, and that was very interesting. It was frustrating at times because we never were really able to define the disease. But for many of these people we could localize it to the very small vessels inside the heart, the coronary microcirculation. At the time that was controversial. Now it's widely agreed upon that some of these patients do have a microvascular problem and there are ways to demonstrate it by cardiac MRI. At that time it was frustrating because there was doubt as to whether there was a problem at all. So it took a while to convince some people there was a problem and it seemed to be related to the circulation in the small blood vessels inside the heart muscle.

SS: It sounds like your research helped prove that.

RC: It did. If you were to ask me what contributions I made I think that was one contribution. I think my work in hypertrophic cardiomyopathy was another area where I made some contributions that contributed to our understanding of pathophysiology and helped to guide management. Microvascular angina is another area.

The third field I became interested in was nitric oxide. In the late 1980s, early '90s, nitric oxide was an exciting field to be in. There was a lot of excitement over what nitric oxide meant to the circulation and what conditions were associated with too little or too much nitric oxide. There were several people that I worked with here that were great collaborators. Arshed Quyyumi was one. He directed the cardiac cath lab, and we collaborated on several studies looking at the relevance of nitric oxide to the regulation of coronary blood flow. Julio Panza was another collaborator. He headed the echo lab at the time. Julio got interested in nitric oxide and the circulation effects on the systemic circulation and disorders such as hypertension. He and I actually hosted a symposium here in 1998 on nitric oxide, and we invited Robert Furchgott to be our keynote speaker. It was four months after our symposium he was awarded the Nobel Prize along with two other individuals. I can't claim that our symposium put him over the top, as the decision was made to award him the Nobel Prize before our symposium, but we didn't know it, nor did he at the time of our symposium. That was exciting. Soon afterwards, Julio Panza left to become chief of cardiology at the Washington Hospital Center and Arshed Quyyumi left for Emory Medical Center in Atlanta.

So there was a nitric oxide period in my career, working with Julio Panza, Arshed Quyyumi and then later on, Mark Gladwin and Alan Schechter. Mark and I probably had the most interesting collaboration into more basic science of nitric oxide biology. We showed that nitric oxide, which is a short-lived gas, can be kept stable by binding to hemoglobin and be transported into the circulation. So it could be transported from the part of circulation where nitric oxide is abundant and be taken to a part of the body or circulation where nitric oxide may be deficient. We showed that nitric oxide has an endocrine effect, meaning it can have an effect at a distance from where it originates. That was exciting and as close as I got to basic science.

That took me through most of the '90s. Then Mark Gladwin left and he's now chief of medicine at the University of Pittsburgh Medical Center. A lot of people that I worked with have gone on to bigger and better things and I stayed behind. I'm glad that many of my collaborators did go on to become chiefs of departments at various places.

SS: Tell me about your patent.

RC: I have a couple of patents. Although I'd love to claim them all as being inclusive to me, they're shared. Mark Gladwin initiated it. He thought of the idea of obtaining patents for medical applications of nitric oxide and nitrite, as we got into nitric oxide research and learned of these interesting properties that had not been described. It didn't cross my mind that obtaining patents would be a smart thing to do, but Mark thought otherwise. It made neither of us rich, but it does acknowledge that we found some unique properties of nitric oxide that had not been described before and were therefore patentable. We still hold the patents and we still receive royalties twice a year from those patents after all these years.

SS: What does the patent do?

RC: They are on the medical applications of nitric oxide and its oxidized products. The medicinal prospects of nitric oxide because it can be transported and therefore it could be administered in one form or another—be transported in the body to a place like the heart where the nitric oxide might be deficient. He and I worked more on the basic principles of that possibility. As far as the actual therapeutic applications, that's still a work in progress. I left that field. Others are doing that now looking for either drugs or biologics that can be given that would boost the release of nitric oxide.

Initially after we published our findings there was a tremendous excitement in various ways to boost nitric oxide in the body. Some of them have turned out to be not promising or very effective. Others, I think there is some possibility of benefit to arteries and the circulation. I left that field, maybe unwisely, after Mark Gladwin left because he was my pillar in the actual basic science, especially the chemistry. There was a lot of sophisticated chemistry in nitric oxide biology. That just didn't excite me as much as it excited Mark. He was really interested in the basic chemistry. I was more interested in the physiologic effects of nitric oxide. My cath lab interest was the coronary circulation and blood flow. His interest was the basic science of nitric oxide and it still is. He's still very much involved in that research in Pittsburgh. I moved onto other fields and left the nitric oxide field behind.

SS: What was your next field?

RC: My final field was somewhat related to nitric oxide, but in diabetes because diabetics have vascular abnormalities and we were interested in what the role of nitric oxide played in diabetic vascular disease, and could we change that if they were deficient. We did show that diabetics have diminished nitric oxide production in their blood vessels. We were interested in what nitric oxide meant to blood flow in their heart and in their systemic circulation, and whether there were things we could do to augment nitric oxide.

I found other collaborators here in the Clinical Center, including Kong Chen and Anne Sumner in NIDDK [National Institute of Diabetes and Digestive and Kidney Disease], who were much more knowledgeable of diabetes than I was. They were knowledgeable of diabetes in general, much more than I was. We had some interesting collaborations. But that line of nitric oxide petered out and I no longer felt I could make a major contribution in that area. That's when I decided, in 2012, it was time to leave that field and I had to decide what I was going to do next.

Fortunately, an opportunity arose in a completely different aspect of clinical research—human research protections—the Institutional Review Board, or IRB. The last stage of my career, the one I'm winding down now is my work with the IRB as its chair. At that time the NIH intramural research program had twelve IRBs, and they were institutionally embedded. NHLBI had one, NCI [National Cancer Institute] had their own IRB and so forth. At that time the IRBs were largely autonomous committees. We all had federal regulations that we had to abide by, but as far as how we applied the regulations, it was individualized. A lot of people saw that as a problem and rightly so, people doing things differently. There needed to be some uniformity of how we approached protocols and how we interpreted various risks and so forth.

At that time there was a new national organization, AAHRPP [Association for the Accreditation of Human Research Protection Programs] that was looking to regulate the human subjects protection programs by a certification process, in an effort to come up with some kind of common standards by which programs, mostly in academic centers, but some in private hospitals, some commercially based, so that there might be some uniformity to how the federal regulations are applied. I came at a time when we were trying to get AAHRP accreditation and that meant a lot of work in codifying our policies for patients of human subjects research. I found that to be interesting.

I enjoyed the human subjects protection side of research. I had been a researcher for many years presenting my protocols to IRBs for approval so that I could begin a research project with human subjects. Now I got to work from the other side of it, but also to appreciate the difficulty in performing human subjects research. I had sympathy for researchers who encountered difficulty in gaining IRB approval of their research, often over matters of minor importance. I feel some pride that I worked with investigators, many of whom are friends, but if not colleagues in a sense at NHLBI, to work with them so that their protocols could ultimately be approved. I tried not to be adversarial. I tried to be collaborative, but attentive to federal regulations and the NIH policies for ethical conduct of clinical research.

Some people saw that—not only what I was doing at NHLBI, but what the other IRB chairs at other institutes were doing—as a problem because of potential conflicts of interest and felt that there should be more of a distance between the IRB and researchers. They felt that perhaps it's better to have a program that is taken out of the institutes so that there is a boundary, there is a barrier between the IRB with their regulatory authority and oversight, and the researchers and their hope of conducting and accomplishing important research that would be important for their careers as well as their patients.

I've been here during the evolution of the IRB structure for the past several years. This is now my swan song: The NHLBI IRB will be disbanded in a couple of months. We merged with the Genome Research Institute's [NHGRI] IRB, because initially the thinking was we could have a structure where instead of having twelve instituted- embedded IRBs we'd have five or six that would be combinations of IRBs and they would no longer be embedded within the institutes.

We were the pilot of this process and we combined with NHGRI. Ultimately the decision was made not to adopt that kind of structure, but instead have a central pool of IRB members and they would be drawn upon to convene an IRB on a given day, and that IRB would have no affiliation with an institute. That's the way it's done at many places and works well. It's different from what we had here and I think it will work. It will be different and our investigators will have to adjust to it.

I wanted to retire and felt this was a good time in my career to say I've run the good race, a great forty years, but it is time to spend more time with my family and do other things.

SS: And also going out, not every doctor could have the brain to do a bureaucratic reorganization, which sounds like the IRB work was.

RC: It was a definite challenge because there were many opinions as to how it should be done and many approaches were considered. We looked at academic programs from around the country and felt that we needed new leadership to guide us. I was part of the search committee that ultimately hired Dr. Jonathan Greene from Washington University School of Medicine, where I did my medicine training over forty years ago. He's now directing the new IRB program [Office of Human Subjects Research Protections]. Tiffany Gommel runs the IRB office [IRB Operations] of people who actually work with the investigators to get their protocols in a condition that is acceptable for IRB review, to get all the documents together and make sure all the approvals at a lower level are obtained before it goes to the IRB. She came from Rochester, New York. The two of them are heading the program and I've worked closely with them up to this point in time, and I will leave wishing them well with this new program. It works in other places, no reason why it shouldn't it work here.

This seemed like a reasonable time [to retire]. I actually live in Florida now. My wife retired from her job at Seven Locks Elementary School in Bethesda. We built a house in Florida north of Orlando where we knew we would ultimately retire. Her mom was living close by and needed some extra help and attention. So we decided that she would move to our house there and we'd sell our house here. She's down there at our home in Florida in a little town called Howey in the Hills. I have an apartment in Bethesda. We sold our home where we lived for thirty-something years. I walk to work. I don't even have a car here. I just walk back and forth to my apartment. Then on Friday evenings I fly down and on Monday mornings I fly back up. People talk about their difficult commutes. I have one of the longer commutes from here to Orlando, Florida, where I keep my car parked in the long term parking lot, spend the weekend down there and then come back up here. My wife said, "It's like dating all over again because I see so little of you." (Laughter.)

That's what we've done for over five years now, but this is getting old, so it's time for me to spend more time with my wife. We want to do things while we're still healthy. We're both almost seventy years old. We want to spend more time with our three grandkids and travel, and do other things, and look into other opportunities. It was never my ambition to die on the job. It may be other's ambitions, but not mine. It's been a great career. I've enjoyed my forty years at NIH.

SS: You hit a couple of milestones that also helped you, forty years, turning seventy. It makes a lot of sense. If you don't mind, I do have some questions. We've gone over some areas that I want to drill into more and this is the perfect place to say wonderful, thank you. There was some other research that you did, postmenopausal in women and heart problems, it seems like you focused on that also, which you didn't mention as part of your research.

RC: That was never a direct focus. I didn't say, "Why do postmenopausal women have heart problems?" It came about because of two areas that I have talked about. One was this problem of chest pain that is difficult to diagnose and treat in some patients because they don't have conventional abnormalities on EKG, echo, cardiac catheterization, the tests that we use to demonstrate heart disease. Most of those tests were normal. I got interested in that because we were being referred patients with suspected cardiomyopathies or suspected heart disease, and we did all our testing and they don't have what's obviously apparent. So I became interested in that problem.

It turns out that many of those individuals were postmenopausal women. Then that led to our discovery that many have a problem with blood flow in their heart, this microvascular problem. That segues into nitric oxide. So we got interested in effects of estrogen on nitric oxide, thinking that maybe one reason why postmenopausal women might have a blood flow problem in the heart is because with the hormonal changes—the precipitous drop in estrogen in postmenopausal women—there might be an effect on nitric oxide production that could be harmful.

We looked into that both in the heart and in the systemic circulation and estrogen clearly has a role in regulating nitric oxide. Then we subsequently learned that it's a redundant role. So just because estrogen levels drop doesn't necessarily mean that nitric oxide levels will go down. It does in some women, but there are other redundancies in nitric oxide biology that for most postmenopausal women, fortunately, their nitric oxide levels remain stable.

We tried to tease out why is it for some postmenopausal women there are drops in nitric oxide, which could lead to problems with blood flood and yet others it seemed to be okay, and that led me to diabetes. Diabetes is common in this country, especially obese adults, and is commonly detected in women approaching menopause and postmenopause who become obese. So it might be that diabetes counteracts the otherwise beneficial effects of estrogen on nitric oxide regulation.

That led me from a research standpoint to obese postmenopausal women who had either pre-diabetes or were diabetic. We showed that indeed that diabetes has a confounding effect and a harmful effect on nitric oxide production and availability to blood vessels in postmenopausal women. We believe that's one of the reasons why obese postmenopausal women are susceptible to arthrosclerosis.

SS: You also looked at hormones?

RC: We did. We looked to see in those women with deficient nitric oxide because of their postmenopausal status whether could we restore the nitric oxide with estrogen. And we showed that was achievable both in the heart and in systemic circulation. We felt excited about that. Unfortunately, the clinical trials looking at the effects of estrogen on heart disease and other conditions were published showing that although there may be some benefit of estrogen, overall there were more harmful effects of estrogen replacement than beneficial effects, and actually more women had heart attacks or died from heart-related conditions and some kinds of cancer with estrogen replacement than without.

The Women's Health Initiative received the most attention, as you know, but there were other clinical trials also showing harmful effects of estrogen. That kind of threw cold water on this notion that estrogen could be the cure-all once a woman reached menopause. Everybody would say use it for symptoms of menopause, but if you're going to use it for symptoms, take the lowest dose in the shortest time possible because long term estrogen seems to have more harmful effects than beneficial effects. Just as I was showing one positive aspect of hormone therapy the clinical trials showed that although that may be true for the circulation in the short term over a period of time estrogen has harmful effects.

It was unfortunate. That kind of put the kibosh on that line of work. Sometimes with research that we do that looks at patients in a snapshot in time can lead you to believe that something is true when over time it turns out not to be true or it turns out there are other confounding issues that negate what benefits you saw. That's a hard lesson that I learned in clinical research that you have to be humble in this business and circumspect about making claims that what you found is the truth when in time others might show you using different techniques or better techniques, or having the advantage of time, find that either it's not the truth, that you misinterpreted what you found or there were other things that you did not measure in your study that have harmful effects.

SS: You mentioned the Women's Health Initiative, were you working directly with that initiative within NIH?

RC: No. That was the extramural-funded program. Gene Passamani, who was head of the Division of Cardiology, had left at that time, but there was interest in studying a variety of women's health issues because there was a concern that women were underrepresented in clinical trials. That led to interest in looking at conditions commonly affecting women—heart disease being one. That led to approval of the Women's Health Initiative, $600 million, something like that. It may hold the record for the most expensive clinical trial done in today's dollars, but [it was] a very important one.

We learned, much to our surprise, that estrogen has its down side. The use of estrogen by postmenopausal women plummeted after the Women's Health Initiative was released and rightly so. It was a hard lesson for some of us who were saying estrogen has these beneficial effects and so forth. Then the clinical trials come out and they say wait a minute, be careful about what you say because it may not necessarily be true when you have the perspective of time and exposure.

SS: That's a good example of that. One thing we haven't talked about is how much contact you had with some of the directors of NHLBI or the scientific directors of NHLBI or directors of NIH, I've jotted down a few names. Was Robert Levy the director of NHLBI when you first came here?

RC: Yes, but he was followed in 1982 by Claude Lenfant. Donald Frederickson was director of the NIH, followed by Jim Wyngaarden in 1982. The scientific director was Jack Orloff and the clinical director was Harry Keiser.

SS: Did you have close contact?

RC: Frederickson and Wyngaarden, never. Claude Lenfant, not so much in my early formative years. A little bit later on I would be asked by him to serve on various committees. Jack Orloff and Harry Keiser, I had much closer contact with. Harry Keiser was the clinical director. He actually directed the clinical research training program that I came under, which no longer exists. He headed that program, so I saw him all the time. He ultimately retired, and sadly, died in 2011, of a progressive neurologic disorder. He was a great person. I very much enjoyed working with Harry Keiser.

Jack Orloff was the scientific director. I highly respected Jack Orloff as a scientist. He was kind of a crusty guy. He was not warm and fuzzy, unlike Harry Keiser, who was. He was just a different personality, but fair. He was the one, as is true for all scientific directors, that doled out the resources. That was important to me in my early formative years. Would Richard Cannon be retained as an investigator or should he just be told go back to St. Louis?

He, along with Steve Epstein, who was the head of cardiology, convinced me to stay. I had gone through some Board of Scientific Counselor reviews and they were very good, so I was put up for tenure and awarded tenure. He was involved in that process because ultimately the scientific director makes that decision along with advice from the Board of Scientific Counselors. I really am in his debt for supporting my early scientific work and ultimately allowing me to stay.

Ed Korn succeeded Jack Orloff as scientific director, who I consider to be a friend. I had more connections with him because of my research work. I found Ed to be a very warm and caring person and he was very supportive of my research. I have the highest regard for Ed Korn.

After Dr. Korn decided to step down as scientific director, Bob Balaban and Betsy Nabel were appointed as scientific directors for laboratory and clinic research, respectively. After Claude Lenfant stepped down as NHLBI Director in 2003, and an interim director for a few years, Betsy Nabel was appointed NHLBI Director. Then she leaves to go to that position. Instead of replacing her with this power sharing arrangement with Bob Balaban, Bob Balaban was appointed scientific director for all research programs. I was appointed clinical director by Dr. Nabel in 2002 and served until 2012.

SS: What did you do in that role?

RC: [I had] a lot of administrative responsibilities. I was responsible for the clinical activities on our inpatient and outpatient units: cardiology, hematology, pulmonary, lipids. I represented the Institute on a variety of committees and was the interface for policies that came down to us from the Clinical Center as far as the conduct of the practical issues or regulations that governed clinical research, not the research itself, but the actual bedside conduct of research within this hospital. That was at a time when we'd moved from the old hospital to the new hospital. It was an exciting time for me to be here during that transition from one place to the other. It was a considerable challenge to move all of our beds and resources from the old brick building to the building that we're in right now.

SS: For some reason I thought that I had read that they were attached.

RC: They are attached. The original Clinical Center was built in the 1950s, but the new CRC was completed in 2005

SS: That must have been difficult.

RC: It took a lot of time. It was time consuming. For me, I worked very hard at being a good clinical director. It did detract from my research because I was spending so much time interfacing with the Clinical Center and this move into the new CRC, and other things that were going on, that it made it difficult for me to partition my research time, so I cut back on my research activities. I was getting into the field of the impact on diabetes of nitric oxide production and I wasn't spending as much time as I wanted to in that field. Ultimately in 2012, something had to go, so the decision was for me to step down as clinical director.

I had the opportunity to become chair of the IRB at the time when the IRB was creating and implementing new policies to get accreditation by the AAHRPP. That was followed by the reorganization of IRBs I discussed previously. I thought that interested me more than research. I had my fifteen minutes of fame in research. So after I stepped down as clinical director in 2012 I became IRB chair.

SS: What was the Medical Executive Committee?

RC: That was my interface as clinical director with the Clinical Center. That was the committee that all the Institute clinical directors serve on that meets with the administrative staff of the Clinical Center and that's where we talk about issues related to patient care and how that interfaces with research. All patients come here to participate in research, but they're patients and we do have to take care of them. Because this is not a full service hospital, that presents some challenges for the adequate care of sometimes very sick patients. So that's where we would talk about solutions to problems: How can we deal with this? How can we make this work? What resources will we need to have either here at the Clinical Center or available to us at other hospitals so that we can safely care for patients at the Clinical Center that are in our research protocols? What research can we do? What kinds of research can we not do because it is not safe or appropriate to do certain research here at the Center? Is it not safe or proper to do here at the Clinical Center? These were the kind of discussions and debates that we would have in the Medical Executive Committee and that was very stimulating. I enjoyed those meetings because there were a lot of problems that needed solving.

SS: Are there one or two problems that come to mind that might be interesting to talk about or something funny?

RC: One of the bigger problems we had was caring for children. We do have protocols that involve children including within our own Institute. And we do have pediatricians in the Clinical Center associated with some of the institutes, but we don't have a true Department of Pediatrics and we don't have the capability of caring for the very sickest of very small children or infants. We don't have pediatric surgical services here. We had to cobble together support services, subspecialty services and anesthesiology to care for very sick children. That took several years of working through that so that we could safely bring children to the Clinical Center and care for them. A lot of that involved collaborations with Children's National Hospital. Those discussions began during my time with the Medical Executive Committee, and continue to this day.

Another was partnering with the military hospital across the street that wanted to get engaged in research, but were constrained by a lot of regulatory restrictions that they had regarding active military people and retirees that we had to work through. Those were interesting discussions. As a result, we've been able to form some partnerships with the Walter Reed National Military Hospital, although it was Bethesda Naval Hospital when I was clinical director.

Another issue that we struggled with that extended from my time as clinical director to IRB Chair was the perception and actual experience of many clinical investigators that it was becoming increasingly difficult for them to get protocols through the IRB process. So I served on a committee with Cliff Lane to investigate the pain points, the pressure points responsible for the length of time for protocol approval. That's one thing that led to the reorganization of the IRBs to establish some harmony into how IRBs worked, how they operated, and what IRBs were doing that were required by federal regulation, and what IRBs were doing that were making the lives of investigators miserable, but were not really necessary. Some IRB actions really didn't appear to serve the purpose of protecting human subjects.

One example was all protocols have to undergo a scientific review before they come to the IRB. Many IRBs would then subject the protocols to another scientific review by the IRB and sometimes make them change their protocols considerably. A lot of investigators felt this was “double jeopardy” and rightly so, and that they were being put through too many hoops to get their protocols approved. It was having a damaging effect on clinical research, particularly young investigators who wanted to get into clinical research but only had a finite period of time before decisions had to be made about their careers, and who were turning away from clinical research. Many were choosing to go into basic research because you could get something started faster. No guarantee that you'd be more successful, but at least you could get the project started more quickly than writing a protocol and having to go through the IRB process.

I was part of that committee and that review. We had a lot of meetings with investigators to try to find out what really troubled them the most, and then we had to come to grips with what's doable and what's not doable. There are some things you cannot change by federal regulation and other things they have a real point there and we need to make some changes.

When I became the IRB chair I was actually in a position to incorporate some of those changes. That was an interesting transition time for me as I had once been on the investigator side of this process and now to be on the IRB side, and see what we could do to facilitate clinical research attentive to investigators’ concerns I mentioned previously. As IRB chair, I tried not to make the protocol review process unnecessarily onerous on investigators. I feel proud that I was part of that process to try to implement those changes.

SS: When you came here, were there IRBs?

RC: Yes. The history of IRBs, as you know, dates from the abuses of human subjects "research" dating from the Nazi experiments during World War II. But also in this country there were several instances of harm to research subjects, the most notorious of which was the Tuskegee experiment. That was actually funded by the U.S. government, by the Public Health Service, to look at the natural history of syphilis in poor black sharecroppers in Alabama, near Tuskegee. At the time that the study was initiated there was no good treatment for syphilis. They included some very toxic compounds, arsenic and mercury, and so forth that had lots of side effects.

As the study went on penicillin became available, especially after World War II, and was found to be a very effective treatment for syphilis, but that treatment was not offered to the participants in this study. So when this came to light in the 1970s, that really prompted a huge outcry, obviously, and it was scandalous. It resulted in many papers being written in prestigious journals about these atrocities and about the need for regulatory oversight of what researchers were doing, and ultimately Congressional hearings and so forth that then led to what we consider the modern era of IRBs.

The Clinical Center actually predated the federal requirement for IRBs in any federal funded research by several decades. The old brick building, the old Clinical Center was built in the 1950s. Shortly after research subjects were admitted here, it was felt there was a need to provide some oversight to research that was being done just to be sure that people were not subjected to unnecessary risks. Interestingly, a lot of the concern in the beginning was for research risks to healthy volunteers but ultimately research patients came under IRB oversight as well. That's something the Clinical Center should be proud of that they have that history of concern regarding ethical conduct of research that predated what was required at the federal level.

SS: You spoke earlier about how the Clinical Center has changed over time. Has it evolved to less patient care?

RC: The numbers of patients in the new hospital, the Clinical Research Center, is less than the high watermark in the old hospital. There are several reasons for that, one of which, at the time that the old Clinical Center was built in the 1950s it was after World War II, and clearly the people were realizing that we have some major health problems now that the war was over, and we had some money to think about it to apply to this. And there needed to be research into conditions such as heart disease and cancer that were now emerging as the major causes of death in the United States. In the early part of the 20th century it was infectious diseases that killed most children and adults. As people lived longer it became chronic conditions including cancer, and heart disease, diabetes and chronic lung disease especially related to smoking that became the leading causes of death in the U.S.

At that time in the 1950s and 1960s academic centers didn't have the funding or the capacity to perform that kind of clinical research. So there was felt to be a need for a federally-funded research hospital—so when the Clinical Center was built in the 1950s that was unique. There just weren't many other places that could bring patients in for research purposes and have it paid for, so that was built. Then the Vietnam War comes along and now we get all these talented researchers coming here to conduct research, and that was really the high watermark. That was when this was a unique resource and there was considerable intellectual firepower to conduct the research.

As time goes on, a couple of things happen, one of which, the Vietnam War ends and therefore the best and brightest don't necessarily have to come to the NIH, they can stay at academic centers to conduct research. Now there was a demand for funding of clinical research at academic hospitals. They said, “We want to be able to perform clinical research, why should our patients have to travel a thousand miles to Washington, D.C. or to Bethesda, Maryland. We want to do it here at Barnes Hospital in St. Louis, or University of Michigan, or University of Virginia, or Mass U General Hospital, wherever.” So Congress funded academic hospitals to build their own clinical research centers, including Vanderbilt Hospital where I did my medical school training, and Barnes Hospital where I did my residency training, and they retained their best and brightest to work in clinical research, so the need for one place in the country to do that appeared to diminish with time.

In recent decades, the Clinical Center has changed its focus to rare diseases or diseases in which there is clearly a need for new therapies or new approaches, or new ways of identifying it, something that has to be unique that's not being pursued at other places or by drug companies. So there was a shift in emphasis on common conditions, including heart disease, to more of a focus on unusual or rare conditions without satisfactory treatment. That is the current focus now.

Also research here and elsewhere is more genetically based to try to identify which people are more likely to respond to a particular therapy, the goal of precision medicine, the term that is often used is to get a better understanding of the disease, who’s going to have a good course, who's going to have a bad course, who's going to respond, who's not, and so forth. What insights can we get based on the genetics of not only the person but the tumor itself or the diseased tissue. That is a more focus than when I first came.

SS: You were talking about seeing patients, doing research, writing papers, giving talks. How did you do all that?

RC: I was very busy, busier then than I am now. I would come here on Saturdays and Sundays and work. I was launching my career and I needed to impress not only Steve Epstein, but my scientific director who decided who stayed and who would move on. I didn't have to impress Harry Keiser so much, but my scientific directors Jack Orloff and subsequently Dr. Korn. I was working very hard, putting in long hours, but enjoying it. It was fun. And I loved to travel to symposiums and conferences. I met fascinating people in many parts of the world, and that was fun for me. That was a very exciting time for me, professionally, but it took a lot of time.

As I got older and my wife wanted me to spend more time with our growing daughters, maybe I didn't spend as much time with my research as perhaps I should have. But I felt I needed a little bit more of a life outside of the Clinical Center. Literally, I was here seven days week. I'd go home at seven or eight o'clock, and rarely had dinner at home. They would have already eaten. I would stay up until midnight either for my own research or reviewing the work of my fellows or reviewing manuscripts because the more recognition you get the more likely you're to get asked to be on editorial boards of journals. I was on five editorial boards of prestigious journals. That means you have the obligation to review all these manuscripts, so that takes a lot of time if you do it right. For a time there I was working the candle at both ends. I clearly did not put as much time into my work in the last two decades as I did the first two decades. But I got to spend more time with my family and get to know my daughters before they left home for college. I tried to restore some balance because I was out of balance the first two decades.

SS: I think it takes a certain kind of person to be able to do all of those skills.

RC: You have to be able to juggle several balls at the same time. You have to compartmentalize, but that's what you do.

SS: Surgeons just perform surgery—who don't work at the NIH. They're not also doing research and writing papers, on editorial boards and that kind of thing.

RC: Certainly at academic centers people juggle many balls just like I do here. I can't say that I had it any harder than if I'd returned to Wash U to pursue research. Clinical research is very difficult. It takes a lot of time and not everybody succeeds in doing it. Some people are very good at it and can juggle all those balls in the air. Others decide this is not for them and they go into clinical practice or they do something else.

SS: You have a lot of publications, close to 200.

RC: I'm very proud of my productivity. A lot of that reflects collaborations that I had with people that I've named, so I can't take full credit for all of it. But I enjoyed my collaborations. I thought that sometimes putting two or three heads together achieved more than just trying to do something completely on my own. Some of that is purely my own work, but most of it is work that I do with other people. I'm very proud of that.

SS: You mentioned Bob Balaban.

RC: Yes, he is the current scientific director. Bob Balaban established a cardiac MRI program here, which was very exciting because it was a new field. When I came here there was no cardiac MRI. There was cardiac imaging, but it was primarily echo-based or cath-lab based. Then he established a cardiac MRI program and brought in Andrew Arai, a very talented investigator to be the clinical head of that program. That became another way to image the heart in a disorder. I was always interested in hypertrophic cardiomyopathy and in patients with chest pain with micro-flow abnormalities. It became a very useful tool for me to use in my clinical research. I had many collaborations with Andrew Arai that were productive and I'm proud of that.

SS: You spoke of mentors and friends who worked here then went on. Why did you choose to stay here?

RC: A couple of reasons. One, maybe it was the path of least resistance from a family standpoint. My daughters were growing up and they were entering high school. So for me to go someplace would have been disruptive. I had an opportunity to go back to Vanderbilt as well as an opportunity to go back to Wash U. So it would have meant pulling up stakes, but people do it in the early careers. Also, there was no guarantee that the grass would be greener on the other side of the hill. I had a good thing going here. I had collaborations that I enjoyed and that were productive. I was writing a lot of papers. I was getting good reviews from the Board of Scientific Counselors. I was traveling to interesting places and giving talks, which I enjoyed, so why move? There wasn't an imperative that made sense to me to pull up stakes and move to someplace else.

That may have turned out to be a good decision or it may have turned out to be a bad decision. I think being exposed to different people in different places can reinvigorate the mind or it can offer opportunities that you don't have available to you here. So I sometimes think: “What would have happened to me had I returned to Barnes Hospital or gone back to Vanderbilt, or gone to other places?” Would it have been a bridge too far and I would have collapsed or would not have had the collaborations or it would have taken me time to reinvent some collaborations that I had already established here. Or would it have jump started my career and provide some opportunities that were not available to me here because we're not the kind of hospital like Barnes Hospital or Vanderbilt, or other places, big hospitals with large numbers of patients, and therefore more opportunities to look at different problems than here where there are a smaller number of patients coming through our system.

I sometimes wonder what would have happened had I gone someplace else, but that's hindsight. I made the decision to stay here and I think by and large it was the right decision for me. I'm not saying it's the right decision for everybody. I think some of my friends left because they thought differently and they thought that there would be better opportunities, not only promotional opportunities to chair a department, which was never offered to me, but then they would have opportunities because they'd be in a big medical center with lots of patients. You wouldn't have to go out to search for patients to participate in research studies.

That's something that's always an issue for us at the Clinical Center, finding people that are willing to come here, sometimes traveling great distances to participate in our research even if we don’t provide transportation. That's why a focus on either rare diseases or unusual problems is a better draw for us because sometimes frankly people don't have many other options and are willing to come here because we're the last hope for some people.

SS: Did you also teach at Georgetown?

RC: I had an academic appointment at Georgetown on the clinical faculty. Yes, I did give lectures in their medical school. I did it for a couple of years. Also I had several residents from Georgetown come and spend time. That was a great experience and I enjoyed that.

SS: What did you teach?

RC: Physiology, cardiology, cardiac hemodynamics, as I did with fellows that came to work with me over the years. I had about thirty fellows that came, some from other countries, others from parts of the United States to work with me in research. I mentored them and that's one thing I'm proud of. If you were to ask me what I'm proudest of that would be one of them. Hopefully I had a positive influence on about thirty trainees who have now gone back to medical centers. Some stayed in research, some have become full professors. Others said, “That's not for me,” and they went into clinical practice or to more clinically oriented kinds of activities.

Most of my teaching for them was clinical research. How do you design a study? How do you identify a problem? How do you approach it? How do you decide what you can do to shed some light on the cause of that problem or improve the management of that problem? How do you think through those steps? How do you put it on paper? Write a protocol that will address those questions in an ethically, scientifically sound manner. That's a lot of work. Writing protocol takes a lot of effort. It's like hearing a nice song and saying, “I could have written that song on the radio, I could have done that.” Well then do it. Try it sometime. (Laughter.) Writing a protocol, I wouldn't put in the same category as a piece of musical art, but it does require a lot of time, effort, and thought, and it is hard to do.

My teaching was how do you do that and how do you write protocol? How do you get it through the IRB? How do you implement it? How do you analyze the data? That was my teaching for my fellows, including people that would come from Georgetown. How do you do clinical research? How do you decide that this is something you want to do, like Gene Passamani talked to me so many years ago when he suggested I come to the NIH for research training. How do you know that you have that itch to do clinical research and how do you scratch that itch? At the time, the late 1970s, it was the NIH, this was the place to be. This was the place to go.

I hope I passed that enthusiasm along to others. I keep in touch with a lot of them. I tell them, send me emails, tell me of any changes in your life, and they do. Many are now married, have kids, and gone to other places. I don't keep in touch with all of them, but most of them I do.

SS: That's impressive.

RC: We had yearly gatherings. We had social activities together, so it wasn't all work. We tried to have fun. We always had a picnic at the end of the year, usually in May when the weather was turning nice and that was a lot of fun.

SS: In the earlier generations before you were here, I heard about the teamwork and the camaraderie of the people working here. I heard Eugene Braunwald give a lecture online [part of the NHLBI 70th Anniversary Lecture Series] about people [who worked for the NIH] and would go to everybody's house or apartment with food and wine, and they would have parties. Was it similar in the '70s as it was in the '50s?

RC: Certainly when I arrived the department was relatively small compared to what I was used to at Barnes Hospital. There were probably forty cardiologists in that hospital. Here there were eight maybe, a smaller group of people. So it was easier to get to know people because the group was smaller. There were a lot of collaborations. It was rare that somebody would do something completely solo. Usually there was some collaboration because we needed the expertise or the technical skills of somebody that could help us with our clinical problem that we were investigating.

We did have socializing. It certainly wasn't every week, but it would be a couple of times a year. There would always be a holiday party and Steve Epstein would always have a party usually May or June when fellows were leaving, at his house. He and his wife Bea were gracious hosts. At the more individual level, I don't know how often people would go to each other's houses. I told you about Bob Bonow, and he and his wife, and my wife and I would do things together. Doug Rosing, he and his wife, we would do things together, but not a lot. There would be a couple of times a year we would have that social interaction. My wife's and my social interaction was outside of the NIH, church or something else. So I wasn't here in the '50s for comparison, so I don't know anything about their social activities.

I do know that Braunwald drove people pretty hard. He was a driven individual. I guess all investigators are to some degree. His fellows were expected to work very long hours at the Clinical Center. Those are the stories that I heard. Maybe just to keep people sane it would be good to have parties once in a while, have potlucks, or whatever they did back then. I can't compare. I can only talk about my era. We certainly got along well together. Yes, there were sometimes personality conflicts. That happens in any organization. It happened in cardiology and it happens to this day. Fortunately it doesn't derail the entire enterprise and overall life goes on, and we move forward. There will be friendships, but there will be interpersonal spats. That has happened in my career and will continue to happen.

SS: I was impressed in my research on NIH how central cardiology is from Framingham, or NHI, to today. And [the research] looking at what individuals can do so that you don't get heart disease. Do you feel it's a central theme to the organization?

RC: Talking nationally or globally about research in heart disease over several decades, my career as a cardiologist has been remarkable. There was not much we could do for patients who had a heart attack back when I was a medical student or even an internal medicine trainee. We had intensive care units and we'd sit there waiting for the arrhythmias and then we'd shock them. There wasn't much more we could do beyond that. Then entered the era, some of which was started by Gene Braunwald and Steve Epstein who followed Gene Braunwald, in trying to think of drugs that could limit the infarct size. Then came the era of reperfusion, [Andreas] Gruentzig then Kenny Kent at the NIH, in opening up arteries, and that became clearly the preferred way to deal with a heart attack, as it restored the blood flow.

During my training you basically held your breath and hoped that patients with heart attack survived their stay in the coronary care unit and didn't have too big a heart attack such that they were in heart failure. There have been tremendous advancements in treating coronary artery disease, in treating heart failure, treatment of arrhythmias and how we approach arrhythmias, how we diagnose heart disease with new imaging techniques. We mentioned cardiac MRI and there is cardiac CT, a big focus of research in our department for diagnosing heart disease.

With the exception of Dr. Morrow's inventing a procedure to treat some patients with hypertrophic cardiomyopathy, the Clinical Center wasn't a cutting edge heart surgery research program. We had our collaborations with the heart surgeons, but ultimately Ed Korn who was scientific director made the decision to close the heart surgery program here in 1990, because he felt that the research productivity beyond the collaborations with us in cardiology was deficient. And it was hard to attract good heart surgeons to come here and pay them what they could get elsewhere and fulfill the mission of the NIH to do research, so he closed the program. I understand why that was done, but it was a big hit for cardiology. We lost cardiologists because they said, "I'm not staying in a program that can't do heart surgery. We need heart surgery to work with us and help care for our patients." So we lost some people at that time.

It made us a different department than was the case when I came in 1979, which was a lot smaller version to what I was used to in my training. By and large we did everything that we could do at Barnes Hospital, just on a smaller scale. Now it's very different. We still make our clinical research contributions but in different a way. When I arrived focus was on cardiac hemodynamics in cardiomyopathies and disorders of the coronary blood flow. Now there's a major emphasis on imaging, both MRI and CT. Now there is research interest in molecular changes that occur during development of heart conditions at a very basic level. [Michael] Sack, who's the head of cardiology, and Paul Hwang are particularly interested in that line of investigation. Also of interest is how certain rare conditions involve the heart and blood vessels, and the molecular mechanisms that account for some rare conditions. Manfred Boehm, among others, is interested in some of those rare conditions.

Sometimes what you learn from rare conditions has application to more common conditions. So people ask why do you want to study rare conditions. That's not the leading cause of death. That's a shortsighted approach to disease. Studying rare diseases can provide tremendous dividends in understanding what can happen in common diseases, both in understanding what causes them, but also coming up with new approaches to treatment.

SS: Did I hear you correctly earlier—did you mention the artificial heart?

RC: We never did artificial heart devices here. That was never a research focus here.

SS: I thought I remember that one had been done here [in my research].

RC: No artificial heart. There was research looking at different approaches to repairing heart muscle using cell-based therapies, and I was tangentially involved in one protocol that unfortunately didn't work out. There was a heart surgeon with an NIH appointment, Keith Horvath who led this effort, and who was interested in xenotransplantation, taking the heart of one species and transplanting it into another species.

SS: Okay, I must have misread that. One thing I was impressed with was all the work and studies that was done here before you came, in terms of prevention to tell about what causes heart problems in the first place. All that was done here?

RC: It was certainly done partly here. What was done here was mostly, as far as the preventative aspects of cardiovascular disease, in the lipid field, understanding the disorders of lipids that can lead to premature atherosclerosis. A lot of that research was done here by people who worked here but then went someplace else. [Michael] Brown and [Joseph] Goldstein, for example, who were at the NIH, left and went to Texas, won the Nobel Prize [1985] for understanding the LDL receptor and its absence in some familial conditions with early onset atherosclerosis and then also led to the development of commonly used drugs called statins to lower cholesterol. As far as preventive cardiology, that was mostly in the lipid field. That was NHLBI, not cardiology per se. We had collaborations with our lipid researchers and I participated in some.

The other contribution of NHLBI in understanding the risk factors was the Framingham Study. That did not involve us here. That was funded by the extramural division of NHLBI. It still is. Actually the funding has been shifted to intramural sources, an administrative action, but it doesn't directly involve anyone at the Clinical Center.

SS: Okay, thank you. You certainly earned lot of awards for your work.

RC: I did, including one yesterday, the Office of the NIH Director Administrative Award [IRB Operations and Reorganization] that was nice. Then I got another Director's Award back in 2000 for my work with the microcirculation of blood flow problem that causes chest pain, sometimes in post-menopausal women. And there have been other awards from NHLBI, several "Bench to Bedside," most of those with Mark Gladwin for my work on nitric oxide, because it gave us some additional resources to bring in fellows and expand our research. That was nice. It's a recognition that we had some ideas that are worthy of additional funding, additional resources, and that it has that "Bench to Bedside." Mark Gladwin was more the bench guy, I was more the bedside guy, so that was a collaboration that worked very well for us.

SS: Do you enjoy working with people?

RC: I do. I try to identify people—and others do this as well—who can help you or you could help with their research. And it's got to be a mutual attraction. You don't want to take advantage of somebody and say, "Please do this for me. I know there's nothing in it for you, but it will help me, would you do it?" So there's got to be something that's mutually advantageous. That's what makes a collaboration work. Fortunately I had that opportunity and I've been able to identify people or people have identified in me opportunities to work together and accomplish much more than we could have independently. That's the nature of a good collaboration. The majority of what I've done has been collaborative research, working with others that compliment what talents I have.

SS: Some prefer the research part and you prefer the bedside part.

RC: I've always preferred the bedside part.

SS: Is that because you enjoy working with people?

RC: Definitely so. At the end of it all, if I'm remembered for anything I want to be remembered for being a good doctor, not necessarily as being a good researcher or a good clinical director, or IRB chair, but for being a good doctor. That's what I wanted to be at the outset of my career, that's why I went into medicine and that's why I enjoy my clinic at Suburban Hospital that serves people that don't have insurance. I like people coming in and saying, "Thank you, Doctor." For me, that still gives me a thrill to have people say that on [cardiology] Consult Service, I get the same reward, people thanking me for taking the time to come in and talk to them, trying to help them out in some way. That's what I've enjoyed the most.

Hopefully if people think fondly of me sometime in the future they'll say, "Yes, he was a good doctor." That's what I want to be remembered for. All the other things, yes, I have varying degrees of pride in, but being a good doctor, if people think I was a bad doctor, that would hurt me. [I would think]: "What did I do wrong," because I really tried to be a good doctor.

SS: Do you have any advice for medical students?

RC: I'm proud that I've had thirty or so fellows and medical students work with me over the years. What I try to instill in them is to find your passion, find what it is that excites you and then immerse yourself in that. I only partly followed my own advice. I pursued many passions and I think for me that seemed like the right thing to do. I don't think it's necessarily great career advice. You should probably find one passion and immerse yourself with it and stick with it, and if it doesn't work out, fold your tents and move on.

I would follow different passions as time went on, so there are definitely stages in my career where I went from one thing to another, transitioned to something else. Even though at the time it seemed like the right thing to do and it was enjoyable for me to do it, in hindsight it might have been better for me to stick with one area of research and immerse myself in that. The difficulty for me is that I was and always have been a clinical researcher, a bedside kind of researcher. I've not been a basic researcher. Don't pretend to be. I tried it, didn't always work out. It worked out better with somebody like Mark Gladwin who knew what he was doing.

In clinical research, it's not easy to stay in one area and just do that for the rest of your life. It really doesn't lend itself to that. You develop skills and techniques and once you solve one problem you take those same skills and you move to another problem because you can use your same skills. Basic research I think lends itself to evolving laboratory techniques that allow you to remain within one particular area of research. You can more easily adopt genetic measurements or biochemical measurements, or assays that can allow you to stay in a field longer.

For me, I'm a more technically-oriented kind of person. I would learn techniques useful in clinical research, such as measuring coronary vasomotion and blood flow, but once I mastered those techniques if I ran out of research opportunities in one direction I would take my same techniques and look at another problem where my techniques could be useful. So once I'd run a good race in one direction, I would head off in another direction.

At the time the Board of Scientific Counselors was agreeable to that. I was able to show productivity in those areas. But it's not necessarily the same advice I would give to someone going forward to move around like that. It's probably better to find your passion, stay within that field, learn all the techniques you can, get the best mentoring you can and then go as far as you can go.

SS: It sounds like one of your passions was solving problems.

RC: That's true for any investigator. If they don't enjoy problem solving, they're not in the right line of work. But that's true for medicine in general. Being a good doctor is being a good problem-solver. Patients present with a problem and you try your best to figure it out or if you can't figure it out what does it take to get the answers. If you can't get the answers then be honest about it and then decide what you do next.

SS: Well, you've been talking about your legacy all the way through it seems, are there any things that stand out that you haven't mentioned today? Or would you like to list them here to see what you've mentioned and not mentioned?

RC: I'm proud of the people that I've worked with. I've worked with some amazing people and I'm proud that I had the opportunity to have that exposure and that opportunity to work with so many talented people here at the NIH, and I've mentioned some names to you. That's a source of pride that I had that opportunity. Many I continue to consider friends. I am proud of the research that I did in the fields and areas that I talked about. Could I have done more? Could I have accomplished more? Could I have found cures that I never found? I guess there's some regret that perhaps I could have gone further and some of my colleagues did go further in various areas that I mentioned over time. I'm envious of them, but not jealous. I'm glad they achieved what they did.

I think a person of my intellectual skills and with my temperament, energy, and ambition, I think I did as well as I could do. I don't leave with a lot of regrets. Everybody possibly has some regrets that they could have done more. It's not something that I obsess about, that I pine over. I think on balance I accomplished a lot and my publications show that. I'll let others decide whether what I did was important or not, but I'm proud of what I accomplished from a research standpoint.

From an administrative standpoint, as clinical director, I did my best. I worked hard at it. How effective was I? There, I have probably some regrets that I wasn't as effective as perhaps I or others wish I could have been. But I did give it my best effort. I'm glad I stepped down in 2012, and I'm glad I had the opportunity to do the IRB side of things. I'm very proud of that activity because that was a major time of transition at several levels. I'm glad I was a part of that and I think I made some contributions to that process.

I'm very proud of the clinic that I started across the street with Gene Passamani and some others for uninsured patients with heart disease, that's completing twelve years of existence. I hope that will continue when I leave. I'm working on that transition, getting people to take that clinic over when I leave, so it doesn't collapse. I think there are a lot of other people that are proud of that as well, if not in NIH, in the community. I'm happy about that.

I'm proud that I had the opportunity to have some influence on the lives of thirty or so fellows that worked with me, many of whom keep in touch with me, some of whom have become academically accomplished, others decided to go into clinical practice. I'm happy for them. And I'm glad they think enough of me to keep in touch over the years. I consider them my legacy in a sense. They're the people that will carry the flame and hopefully will make accomplishments of their own, big and small, and they will have a passion for the bedside part of it as I always have, and being a good doctor. I tell them: "If you do nothing else, at least be a good doctor. If research doesn't pan out, that's okay, scratch that itch and move on, but at the very least always be a good doctor."

SS: What do you mean by a good doctor?

RC: Someone who takes patients that have a problem, they're sick, they have a condition and they come to you for help, and taking them seriously, focusing your attention on them, showing compassion and empathy. Because but for the grace of God, our places could be reversed, and how would you want to be treated if our roles were reversed? Perhaps one of these days our roles will be reversed as I get older. You want to be treated with compassion, with empathy. You want to be that patient's entire focus. You don't want to thinking about other things while you're talking with them. You want to show that you are really working as hard as you can to help with their problem so that based on your experience or what you've been taught and trained to do, if you can't tell them face to face at that point in time what you can do to help them, and sometimes I can't, then what are you going to do next? What is your plan to go forward either with additional testing or to get additional input from other people that you think might have expertise that you lack.

So come up with a plan so that they're engaged in this plan to deal with their problem and that you don't show arrogance in your thinking. We have a discussion and you are part of that discussion, and, "Here's what I think, but let's talk together about what we're going to do to solve your problem." I think that's part of being a good doctor. Once you engage and embark on that path that you mutually agree upon is the reasonable way to go, you don't abandon them, you don't leave them. You stay with them. If there are bumps in the road you hold their hand through that problem, that detour if you will, that cul-de-sac. You should try to get back on the main road.

Ultimately when you've done all you can do or you've treated their problem, it's no longer a problem, and it's time to say goodbye then you say goodbye. That could be a sad goodbye; it could be a happy goodbye. Sometimes it's a sad goodbye that people will die of their problem. We've run the good race and fought the good fight, but there's nothing more we can do. You have to make sure that they know that you're not going to abandon them, you'll be there for them, and that sometimes happens.

I've had to tell people that there's nothing more that can be done with end stage heart failure or hypertrophic cardiomyopathy, which can be a deadly disease. There are people including young people that die of that disease and you have to be able to say in a compassionate manner what they can expect. That's also part of being a good doctor. You can't cure everybody, you can't help everybody, but you give them information that they need to make plans in their life. That's part of being a good doctor as well. It's not one of the happier parts of being a good doctor, but that's part of it.

Then, staying on top of your game. Don't become obsolete. That means reading journals, attending conferences, and staying on top of the latest developments so that what you tell people represents the best information that's currently available. Two or three years from now that may not be the best available. There may be even more information that's available. But you need to be in a position where you can give people the best information available. If you're not in that position you tell them so or you don't do it. That's part of being a good doctor.

SS: It seems to me that you are, indeed, a very good doctor.

RC: Thank you. You haven't put me to test yet. (Laughter.) But if I'm remembered for one thing, I hope that's it. At the end of the day that's what excites me the most is meeting people and living through their experiences, and trying to solve their problems. That's what I've tried to do.

SS: That's what you wanted to do.

RC: From the get-go.

SS: Anything else?

RC: I appreciate Dr. Balaban's invitation to participate in this interview. I think he regrets that Martha Vaughan didn't have an opportunity to share her story. He's doing the right in trying to identify people who have been here a long time to get their experiences recorded and on paper. It's our history. So many people have left where that's no longer possible, so you lose that part of our culture. What you're doing is tremendously important and I thank Dr. Balaban for thinking enough of me.

SS: Thank you very much.

[End of interview]